ON

HE CONSTITUENTS OF FOOD,

AND THEIR RELATION TO

MUSCULAR WORK AND ANIMAL HEAT.

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I. INTRODUCTION.

The rational theory of nutrition commences with the discoveries of Mulder in the year 1838.

In the establishment of the fact that the albuminous matters of plants and of animals agree in composition a foundation was obtained on which observers might safely build. Each subsequent investigation started from this proposition : plants prepare the albuminous matters of animals—and with this were closely connected the numerous questions respecting the theory of nutrition, and the metamorphosis of matter, with which thenceforward physiologists occupied themselves. It was tacitly assumed that the animal organism can form no albuminous matters, of which formation the grounds are even still wanting. In the supply of albuminous matters, therefore, a condition of animal life was found.

But it was known, and this fact received further confirmation, that vegetable food is relatively poor, while animal food is rich in albuminous matter. And, inasmuch as whole classes of animals live exclusively either on the one or on the other, it was evident that animal life may be maintained with little albumen, but that it may also use much albumen.

What was true in this respect of man ?

The structure of his body, and not less his inclinations, wherein the conditions under which our race is laid are manifested, indicate for him a place between herbivorous and carnivorous animals. Both kinds of food, it was inferred, were destined for his use. Two circumstances, however, could not escape attention. In the first place, that whole classes of our fellows are doomed to be vegetarians; in the second place, that in these classes we find least of all examples of the bodily and mental perfection of which we think our race is capable. Comparative considerations appeared further to justify us in seeking the cause of this inferior development, in part at least, in an imperfect supply of albuminous matters. The desirability, nay the necessity, of a more liberal supply was now strongly insisted on. Particularly the want of strength, or rather the slight power of work or energy, was referred to the deficiency alluded to. The fact was adduced that the muscles which perform the work consist for the most part of protein matters, and the metamorphosis of these muscles was looked upon as a necessary condition of their energy. Liebig—but Liebig alone—went, in his zeal for striking antitheses, even so far that he deduced all mechanical power directly from the use of organised albuminous matters, leaving to the fats and so-called carbo-hydrates only the inferior part of serving as heat producers. To such perilous propositions physiologists have, however, not committed themselves. It is indeed true, that we find in Bischoff and Voit, whose investigations were from the first animated by Liebig's spirit, still a feeble echo of the same doctrine; but we must be strangers in the domain of physiology to dispute the point with them, as if it had obtained general acceptance.

Of late years the principle of the maintenance of energy, so fruitful in its results, has been more and more applied to the phenomena of living nature. Moreover, new and important facts have been brought to light, which exercise a very essential influence upon our views respecting the relation of muscular work and of heat to the metamorphosis of matter. I refer especially to two, namely, the cooling of the blood in the lungs, and the slight increase of the urea in the urine, after the performance of muscular work. The first is important in its bearing on the theory of respiration, and on the place where the heat is produced. The second led to the conclusion that in muscular work the tissue is not wasted, but only gives the impulse to the metamorphosis of non-nitrogenous bodies, by transferring oxygen to them (M. Traube). One of our fellow-countrymen ventured still further, publishing a pamphlet, which leaves the impression on the reader that the use of animal food is to be considered, if not injurious, at least as rather superfluous for man.

It appears to me that such a statement should not be left unanswered. It is true that in general it is scarcely necessary to combat an error in the domain of science. Left to itself, it will almost always soon be forgotten, and so dies a natural death, which is also the strongest security against its resurrection. In practical life it is otherwise. Here tradition keeps itself, through incompetent, still more than through competent judges, alive from mouth to mouth. Here it may therefore be expedient, if possible, to stifle in their birth incorrect ideas where they threaten to creep in. Let, however, no controversy be expected from me; I wish, without entering at large into the above doctrine, simply to record my own views upon the subject. Independently of what has been written by Traube and by Beins, it was certainly not unimportant to consider what idea respecting the origin of muscular work and heat the present state of science justifies. To this the following pages will be devoted.

II. BALANCE OF ENERGY IN THE ANIMAL BODY.

Never was a law received with such general approbation as that of the "indestructibility of force." When it was formulized, it appeared as if men had already been acquainted with it for years—had lived, thought, and worked under its influence. It forced itself upon us as a logical necessity. Poor in decisive proofs, it stepped into life; but we had intuitively the conviction that no facts would refute it, and it was used, forthwith, almost rather as a test of the correctness of observations than that it was thought necessary to check it by fresh investigations.

In the first contributions of J. R. Mayer,[a] in 1842, the principle is already fully expressed. Mayer adopts forces in the sense of *causes*, and starts from the fundamental proposition—causa "*æquat*" effectum. Therefore, he says, the effect is in its turn a cause, and herein lies its "indestructibility." But its form may alter, and it is thus "changeable." Mayer's treatise comprised the indication of the chemical energy, which in the combination of bodies is manifested as heat, a correct description of the relation between heat, motion, and gravity (a lifted load); lastly the idea of mechanical warmth-equivalent, which he also already expressed in "gravity"—in "kilogrammeters."[b]

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[a] Annalen der Chemie und Pharmacie. Bd. xxxii.

[b] A kilogrammeter is the *work* necessary to bring a kilogramme (2·2055 lbs. avoirdupois) to the height of a mètre (3·2808992 English feet). By this work we have obtained a kilogrammeter "gravity" as energy. While the word "force" had in mechanics a definite signification, according to which it might be stated as a pressing weight, in kilogrammes, the words *work* and *energy* have been adopted in the first mentioned sense, instead of the word "force" still used by Mayer. A second measure, whereby to express it, is that of calories: a calorie is the amount of heat necessary to raise the temperature of a kilogramme of water by 1° C. (1·8° F.). Now the mechanical equivalent of heat is expressed by the number of kilogrammeters, which are the equivalent of a calorie, that is about 430; that is to say, that a calorie is used to raise one kilogramme to the height of 430 metres, and that, *vice versa*, a kilogramme, falling from the height of 430 metres, can produce one calorie. We easily obtain a just idea of work and energy from the measure with which they are measured. Any one who desires more exact ideas respecting this fundamental matter, may be referred to Helmholtz (Ueber die Wechselwirkung der Naturkräfte: on the Reciprocal Action

Mayer's essay passed unobserved by many; our highly esteemed colleague, v. Rees, immediately perceived its importance, and directed the attention of his friends to it. It was now easy to apply it to physiology. At the close of a public lecture,[a] delivered in the Winter of 1844, I remarked, that it was not sufficient to deduce only the heat developed from the metamorphosis of matter, but that in addition some other force (say work) must have its cause therein; and that only when neither electricity, nor light, developed by some animals, nor mechanical power, which proceeds from all, was lost externally; when therefore all were changed wholly into heat, could the quantity of heat developed correspond to the oxidation of the organic matters; finally, that these matters had been developed in plants under the use of the sunlight, so that we properly find, in the vital phenomena, the equivalent of the light used. All this, and much more, we find accurately and clearly set forth in the pamphlet soon after published by Mayer:—*Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel.* Heilbron, 1845.

Even earlier we find the principle of the maintenance of energy, without being as yet expressed, here and there correctly applied. When, for example, the efficacy of nerves and muscles as a source of heat was assumed, it was generally supposed that this efficacy had required as much chemical action as would have been necessary for the immediate production of the same heat. Liebig had even stated, that the heat which our muscular action can excite outside the body by friction, is as much a product of the metamorphosis of matter as the heat developed in the body itself. But no one was found to be faithful to the principle, and many a proof of inconsequence might be adduced.

Even in 1822 Dulong, and shortly afterwards Despretz, had compared the heat developed in animals with the oxidation in the body, and they had arrived at the result that at least the greater part, 0·7 to 0·9, of the heat given off can be deduced therefrom. In the calculation it was supposed that in organic food the oxygen is combined with hydrogen, and that the remaining hydrogen and carbon with the oxygen taking part in the combustion still develop as much heat as if they had been present in the elementary state. Thus for the burnt carbo-hydrates only so much

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of Natural Forces. Königsberg, 1854), and to Bosscha (Het behoud van arbeidsvermogen in den galvanischen stroom: on the Maintenance of Energy in the Galvanic Current. Leiden, 1858),—in which two writings the profundity of the subject has not suffered from the popular mode of treating of it. We may here further remark, that we have to distinguish between energy at rest, *potential energy*, Rankine (a raised load which can fall), and energy in motion, *actual energy* (a body in motion, *force vive*). What both can perform is *work*, of which the result may in its turn occur as energy.

[a] Blik op de stofwisseling als bron der eigene warmte van planten en dieren. (Glance at the metamorphosis of matter as the source of the proper heat of plants and animals). 8vo, Utrecht. January, 1845.

heat was brought into calculation as the carbon therein existing, in the free state, would have developed. Helmholtz[a] proved the incorrectness of this statement. The heat arising from the combustion of alcohol had been known from the investigations of Favre and Silbermann, and a simple calculation showed that the alcohol alone, although formed with development of heat from sugar, in combustion produces more heat than the *carbon* of all the sugar which had served for the formation of alcohol would be able to do in the *elementary state.* By this consideration the deficit of Dulong and of Despretz was explained, without any necessity for assuming, with Liebig and with Mayer, a greater production of heat in the combustion of carbon in the elementary condition. It should be observed, that in the experiments of Dulong and of Despretz, the animals were at rest, and, so far as they might have moved, this movement here also was converted into the measured heat. According to the principle of the maintenance of work, only this, therefore, needed to be capable of being deduced from the chemical metamorphosis.

A few years before Mayer had expressed the great principle, physiologists had occupied themselves much with the balance between the matters conveyed into and out of the body. If neither in this manner did they penetrate into the proper laboratory of life, they obtained a starting point for further investigation: from the knowledge of the composition and of the quantity of the excreted matters, they ascended to the question, where and from what compounds were they formed, and by this they were led to analyses of the blood and of the tissues, which gave the answer to many a question.

In reference to the energy, the same task had now to be fulfilled. Here also it was necessary to begin by making up the balance. A certain quantity of chemical energy was introduced, under the form of organic food and oxygen, and the equivalent of what of this was used, is found again in the heat and in the mechanical work proceeding from the body. The two balances, that of the matter and that of the energy, form a whole. The thing is simple. The ingested and the egested elements are the same. Their quantity too has remained unaltered. Only the forms of combination differ, and with them the energy. In the combustion of what is taken in we obtain, in fact, much more heat, than in the combustion of what is excreted: it is the difference in their energy. Now to this difference corresponds exactly the sum of the heat developed in the body and of the heat-equivalent of the mechanical work yielded externally by the body.

If we know the value of the three factors mentioned the balance is ascertained. Other forms of energy do not enter into it. The light, which presses into the eye, the vibrations communicated to the auditory nerve, may physiologically be of great importance, quantitatively they are of none in our balance. To electric currents also, coming from without, the body is not, under ordinary circumstances, exposed, and the heat, which we, directly or indirectly, receive from without, remains heat, and passes as such (free or combined) again away from us. Thus nothing else is introduced than the chemical energy of our food and of the oxygen. And, as to the outgoing energy, we have, except what remains in the excreted matters, to do exclusively with heat and with mechanical work. Indeed, no perceptible loss of electricity takes place in man, and the so-called animal magnetism is a suggestion of ideas, no natural force.

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[a] Encyclopädisches Wörterbuch der medicinischen Wissenschaften. B. xxxv. Berlin, 1846. Art. Wärme.

But this balance having been ascertained, our task is not yet complete; it is only marked out. In the body numerous metamorphoses take place, which we must endeavour to follow step by step. But it would be quite complete had we succeeded in tracing the origin of the heat and of the mechanical work from the chemical energy, mediately and immediately. A wide field lies here before us, in which the seed begins here and there to shoot up, but the harvest is still remote.

III. DIRECT AND INDIRECT PRODUCTION OF HEAT.

Heat is in general the final product of change of energy. What occurs in nature as heat can only in part return to another form, and what cannot be communicated to a colder body remains heat for ever. Thus it is too in the animal organism. Just as water, carbonic acid, and urea are final products of the metamorphosis of matter, heat is the last form which the energy assumes. The heat which has once arisen in the body must remain heat, because it has no colder parts to warm. It is the excretum of the energy, partly disappearing as free heat externally, partly existing combined in the excreted gases and in vapour.

Heat arises in the body directly or indirectly, that is either as the immediate result of chemical action, with the sacrifice of chemical energy, or from other forms of energy, which equally were the result of chemical action.

Formerly the direct production of heat was assigned a prominent place. It was admitted that in the lungs, by chemical combination of the oxygen with constituents of the blood, much heat was developed, and it was supposed that, in spite of cooling through warming the air and the development of gases and watery vapour, the blood here assumed a higher temperature. According to this idea we might consider respiration as "serving to the production of heat;" and it was also not absurd to regard some constituents of the food as respiratory food. But when accurate determinations of G. v. Liebig and of Gavarret showed, in opposition to all earlier observations, that the arterial blood is colder than the venous, the chemical theory of respiration, which through the investigations of Magnus had already begun to give way, seemed to have lost its last support. By the fact mentioned, however, development of heat in the lungs was not excluded: it was seen only that the production is less than the loss. Now the loss is, especially in cold dry air, very considerable; for not only does carbonic acid escape in the gaseous state, but the inspired air is also warmed to nearly the temperature of the blood and at this temperature almost saturated with moisture. If the heat developed here cannot counterbalance this loss, this does not show that heat is not produced. In the first place, the mere solution of the oxygen taken in would afford heat; but, moreover, we may readily assume, that even the loose chemical combination with the colouring matter cannot take place without the development of heat. That in the lungs we have not to do simply with a physical change of gases has indeed, been satisfactorily shown by more than one investigation, carried on in the school of Ludwig. One of the latest, that of Holmgren, affords even the direct proof, that the carbonic acid is expelled from the blood by the oxygen, not as if a vacuum, free from carbonic acid, were in question, but actively; and this is scarcely conceivable without the intervention of a chemical action. What is more, an investigation of Sachs, has recently contributed fresh reasons for the formation of carbonic acid, without the tissues, in the blood itself.

On the other hand we know that, in more than one way heat is indirectly produced. In all parts of the nervous system electro-motor action arises, which is not without influence on further chemical change, since it may even make the dissolved oxygen active and thus promote oxidation, while it also undoubtedly, under the influence of the great resistance presented by the badly conducting tissues, is in great part changed into heat. On irritation, according to Valentin, the development of heat increases in the nerve itself. Also in the muscles electro-motor action gives place to heat. Moreover the giving way of the elastic tension, which remains after each contraction, must be found again as heat (compare the following section). Further, the amount of heat is very considerable which arises from transformation of mechanical work of the muscles. I refer, with respect to this point, in the first place to the heart, the most active of all muscles, producing in twenty-four hours a work of about 86,000 kilogrammeters, which, wholly changed into heat, can warm fully 200 kilogrammes by 1° C., and therefore all the parts of the body by 3° C. (5°·4 F.). The work of the heart, at first in part transferred to the elastic vascular walls, causes the movement of the blood, and the friction connected herewith, consumes the tension, to replace it with heat, which is also the case, in so far as this tension forces the constituents of the blood through the membranes. And if by the friction in the capillaries the oxygen united to the blood-corpuscles may be made active,[a] which I have long thought probable, we shall, in its union with other matters find again the work connected therewith as further product of heat, or under another form. I refer, moreover, to the movements of respiration, which are, it is true, slight in comparison to the energy which each time here arises and disappears as tension of the muscles, in order to give place to heat, but which, just like the action of the heart, never cease, and by friction of the air passing in and out, and friction of the passively moved parts constantly convert mechanical work into heat. The same is true of gastric and intestinal movement, and other involuntary contraction of muscles. Finally, in all transitory muscular action, friction must arise, both in the muscle itself and in the articulations, and in general in the passively moved parts; and thus, with consumption of mechanical work, heat is produced.

We may, on the ground of all this, safely assume what may also be more accurately proved by calculation, that the bodily heat is for the most part developed in and through the muscles, principally, as has been said, indirectly, but, as it appears, also directly from the chemical action. This last holds good, in fact, for all parts of the body. Everywhere, where there is capillary circulation, is arterial blood changed into venous, oxygen is chemically combined; and if neither elastic tension nor electromotor action supervenes, we are justified in inferring the direct origin of heat. Indeed in the high temperature of the blood of the hepatic veins (G. von Liebig and Gavarret), and of the saliva secreted under nervous irritation (Ludwig), we have the direct proofs of development of heat in non-contractile parts. And as in continued muscular work the circulation of the blood and respiration are more active, and evidently a greater quantity of blood is conducted through all organs, we have to expect everywhere an increase of the direct development of heat: in all irritated parts this increased activity manifests itself, after continued work, in the increase of irritation, in pain and swelling.

The conclusion is, that both in the blood and in the tissues, heat is produced directly from chemical energy; but that in addition the electromotor action in nerves and muscles, the energy of the elastic tension of these last, so far as it is not utilised in mechanical work—and lastly, a considerable part of the mechanical work itself are changed into heat.

IV. PRODUCTION OF MECHANICAL WORK.

As our muscles contract they are in a state to perform mechanical work. This contraction is the result of a change in the molecular condition, which depends upon chemical metamorphosis. At the moment when this change occurs, the corresponding contraction is not yet present. The change begins by producing a state as if the muscle was extended, a state wherein the energy consists in the form of an extended elastic body. The contraction itself now follows directly. In this sense Weber is right when he deduces all work of the muscles from elastic forces, which, in their turn, are the result of an altered molecular condition. A correct illustration of what happens here is supplied by a load hanging to a string which becomes shorter by imbibing moisture. The weight is gradually raised, in consequence of the greater elastic tension, which is itself the result of the molecular condition altered by taking up water.

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[a] Experiments of Saintpierre, Polytechnisches Journal, B. 162, H. 3, May, 1864, which have reached me while these sheets are passing through the press, seem to prove that mechanical friction really ozonises the oxygen of the atmospheric air.

The elastic tension of the contracted muscle cannot, however, be entirely converted into mechanical work. If the load is infinitely great the muscle maintains its original length, notwithstanding the altered molecular condition: of the elastic tension produced nothing is then used for mechanical work. Could the load be infinitely small, the work produced would be, though the load were moved, likewise infinitely small. In both cases, therefore, all mechanical work is wanting. But in any other loading also elastic tension still remains over at the end of the contraction, which is not converted into mechanical work—in general the more the greater the load is. Thus, by removing the load in parts, we see what remains each time rise somewhat higher, and thus with diminution of the tension mechanical work is developed; but finally, even on account of the weight of the parts of the body, elastic tension which can no more be utilised always remains over. The rule is, that the load is put off at a definite height, and as, at the same moment, the muscle relaxes, the just disappearing tension must appear under another form. This can hardly be any other than that of heat. It thus appears that by the relaxation energy is sacrificed, in this sense, that it is not used for mechanical work. But this relaxation is the *conditio sine quâ non* for the continuance of the work. In the relaxed condition alone is it possible that, by a fresh contraction, work should be anew performed. If the muscle continues tense the work ceases. What we here see is perfectly analogous to what the steam engine exhibits in the rising and falling of the piston. In this case, too, the tension remaining in the steam after the raising is sacrificed, and is, as heat, transferred to the cold water which condenses the steam. Now the piston can also again ascend, just as the relaxed muscle can again perform work. But as little as it can be called work when the piston is held above by unaltered tension of steam, so little is permanent contraction of a muscle, which holds a load only at the same height, to be considered as work. Mayer had already fully understood this. Strikingly he says:—"The performance of a man who with great effort holds a weight free, or stands for hours quite unmoved, is exactly—*nil;* the same, or indeed much more, can be accomplished by a wooden statue." Béclard[a] calls the tension which makes equilibrium with a load, without moving it, *statical;* the movement *dynamical* muscular action. To this distinction we must strictly adhere. Superficially it appears something strange that the tension which is necessary for bearing a load at the same height is not to be called work. The muscles are thereby wearied as well as by raising a load, and the fatigue makes us involuntarily think of and believe in the performance of work. The strangeness, however, immediately vanishes when we take into consideration that in the statical action also work in the muscles is by no means excluded; we say only that there is now no work externally performed, and that we, therefore, must rediscover the whole equivalent of the chemical change which occurs in the muscles, in the body itself, at last under the form of heat. *A priori* we have even no right to set down the metamorphosis as less in the muscle which has only to support a load than in that which has to raise it. If in the first case the mechanical work is wanting, so much the more heat might be developed. In fact, Béclard found that the skin on the biceps muscle increases in heat in statical more than in dynamical action, and he thought that this difference might be brought into connexion with the equivalent of the work performed in dynamical action. *A posteriori* it appears, however, that in the muscle the change is actually less when no work is performed externally. Béclard's result is explained by this, that in dynamical action the circulation in the muscles is brisker, and the blood flowing in great quantity carries away the heat which has been developed from the muscle.

The great difference in the metamorphosis in statical and dynamical action is sufficiently evident from their influence upon the factors of the metamorphosis of tissue. Many years ago I instituted investigations upon this point, of which I have as yet published only the results.[b] Among these is the fact that in hoisting or lifting a load the pulsations of the heart increase much more in frequency than in mere holding, although the same power which extends the muscle in the first case acts only alternately, while in the second it acts incessantly. The influence on respiration, too, is known. "During a great effort," says Mayer, "it is usual to hold in the breath in order to obtain from the chest a firm support for the motor organs; but with this, however, an increased consumption is not yet necessarily combined. In cleaving wood, going up stairs, &c., no one will be tempted to hold in his breath."

In connexion with statical and dynamical muscular action I formerly, also, investigated fatigue. I found that this is not the same in both cases. The fatigue dependent on long-continued work yields only slowly; it depends on consumption of matter, and probably, at the same time, products of the metamorphosis of tissue are accumulated in the muscles and nerves. The fatigue caused by holding a load, on the contrary, speedily disappears, even if it were pushed nearly to extremes; and we have, in order to explain this, to think rather of impediments to the circulation, in consequence of pressure on the vessels in the tonically-bent muscle, than of diminished abstraction of products of metamorphosis—perhaps[a] also of pressure on the nerves. Even when, as a result hereof, the feeling of fatigue still continues, the muscle has again lost[b] its greater extensibility, which remains for a long time after continued work, and therefore its physical fatigue.

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[a] De la Contraction Musculaire dans ses Rapports avec la Température Animale. Paris. 1864.

[b] Verslagen en Mededeelingen der Koninklijke Akademie van Wetenschappen. 1859. D. ix., p. 113.

A muscle which contracts independently, and thereby moves a load, presents, at the first glance, something mysterious. It is not strange that in former times an attempt was made to explain this by assuming the existence of an equally mysterious force. Now, however, we know that a direct solution of such questions is not to be thought of, that we must have recourse to indirect modes; in other words, that we must investigate all that is in any way connected with the phenomena without previously asking when and how the solution is to follow. And certainly of late years physiologists have not been remiss in this direction. The structure of the muscular fasciculi, the termination of the nerves in the muscles, the elasticity, the production of heat, the electrical phenomena, the sounds even of the muscles have been investigated in the conditions both of contraction and of rest; and in order to explain the mutual connexion of these phenomena an attempt has been made to determine, with accuracy the moment of the rise, and the relative intensity of each phenomenon. The dependence on nervous action, the propagation of this action to the nerves, and further to the muscular fasciculi, the chemical composition of the muscles, the products of their metamorphosis, the influence of contraction on these products, even the blood returning from the muscles, have all been subjected to examination. Many important facts have in this way been brought to light. But the peculiar change of the molecular condition of the muscle, which determines the contraction, is still involved in obscurity. Of the connexion of the phenomena we may give the following sketch:—

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[a] According to Leber (Zeitschrift f. rat. Med., xviii., p. 262), the irritated (not the resting) muscle is fatigued by mere extension; the fatigue is therefore determined not exclusively by the work performed.

[b] The mode of investigation, formerly (*l. c.*) indicated by me, may be found more fully described in Dr. van Mansvelt's dissertation, Over de Elasticiteit der Spieren (On the Elasticity of Muscles). Utrecht. 1863. [The English reader will find a translation of the description of this method in a notice of Dr. van Mansvelt's work, drawn up by me for the British and Foreign Medico-Chirurgical Review, Vol. xxxiii., p. 439.—TRANSLATOR.]

In normal life the muscle contracts under the influence of the nerves. The action herein excited (voluntarily, automatically, or by reflexion), manifesting itself in a modification of the electrical phenomena (Du Bois-Reymond), is propagated with tolerably great rapidity (amounting, in frogs, to about twenty-eight metres [nearly ninety-nine English feet] in the second—Helmholtz), and, as it seems, with increasing intensity (Pflüger) to the so-called primitive bundles of the muscles, which are to be considered as the terminal organs of the motor nerves. In the condition of rest the muscles have, with very slight tone, persistent elastic tension (Weber), and with consumption of chemical energy by oxidation, an electromotor action is developed (the cause of the "resting muscular current" of Du Bois-Reymond), and thus mediate, perhaps, also, immediate heat is produced; the muscular sound, too, appears to be present even in rest. The contraction (thickening with shortening), of the so-called primitive bundles proceeds from the points where the nerve fibres are attached with flat expansion mediately or immediately to the muscular substance, and is thence propagated to both sides in each fasciculus under the form of waves, so that a muscular fasciculus is not equally shortened throughout its whole length—in cold blooded animals with the rapidity of about a metre in the second; at the same time, too, the tone of the muscular sound rises. The shortening does not commence directly upon the arrival of the exalted nervous action; there is a latent period of about $\frac{1}{100}$ of a second (Helmholtz), with the commencement of which (von Bezold) an important phenomenon coincides and announces the approaching contraction: this phenomenon is an instantaneous electrical discharge (Meissner) lasting less than $\frac{1}{1000}$ of a second (von Bezold), comparatively weak, but still, probably, equivalent to that of the electric organ of fishes.[a]

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[a] I here follow Meissner's view without assuming it to be proved. The case is briefly this—Meissner had found that on artificial compression of the muscle in the direction of its fibres a negative deviation of the needle occurs—the same phenomenon which is observed in active contraction of the muscle. He now thought that in the latter case, too, the change of form, with compression in a definite direction, might, in part at least, be the cause of the deviation. That Du Bois-Reymond had many years previously investigated the influence of extension and compression of the muscle on the force of the current, and announced as his result, that in compression "we most frequently see the current diminish," appears to have escaped Meissner; however Du Bois-Reymond was far from seeking therein the cause of the "negative deviation." In order to test Meissner's meaning, von Bezold determined (from the secondary action in a second muscle), the moment of origin, and also later, by comparison with the effect of artificial currents, the duration, and the course of the electrical change on the surface of a muscle after irritation of the nerve by an unclosing induction stroke. From this investigation it appeared that the change of electricity here occurring, consisting of sudden diminution and restoration, takes place immediately on the arrival of the stimulus in the muscle, and passes off in less than $\frac{1}{1000}$ of a second. As the muscular contraction does not begin until after a latent period of $\frac{1}{100}$ of a second, von Bezold correctly inferred that the change of electricity observed, as preceding the con-

On the occurrence of the shortening, the temperature of the muscle suddenly diminishes (Heidenhain and Solger, Thiry and Meyerstein), probably in consequence of the greater specific heat of the contracted muscle[a]; but in reality heat is developed (Becquerel and Breschet, Helmholtz) which, with tetanus lasting some seconds, in spite of the greater specific heat, makes the temperature of the muscle rise above what it originally was. The rise continues after the cessation of the contraction (Thiry and Meyerstein), which is to be ascribed, partly to the return of the original specific heat, partly to the yielding of the elastic tension, further, to the descent of the load just raised by the muscle, and probably to still continued stronger oxidation. The original cooling is greater the greater the shortening (Thiry and Meyerstein); the production of heat appears, on the contrary, to be about proportionate to the work of the muscle performed (the same).

A second phenomenon, connected with the contraction, is the modification in the electrical tension, which manifests itself on the galvanometer as diminution of the muscular current, and is therefore called "negative deviation." This continues so long as the state of contraction lasts; so long, also, as there exist on the surface of the muscle rapid alternations in the tension, proved by the secondary tetanus of a muscle, which by its nerve is in contact with the first. The negative deviation may be

traction, cannot possibly be the result of it. But is the change of current actually the same, which in tetanus produces the known negative deviation of the needle? Is it the disappearance and the return of the current in the muscle at rest! Meissner thinks this may be denied. The change of current, determined by von Bezold, he considers as a positive discharge, such as in tetanus is said rapidly to follow one another. His principal proofs consist in this, that in some few induction strokes of the nerve in the second, which are still insufficient to produce tetanus, in place of a negative a weak positive deviation of the needle is seen; and that also in tetanising the negative deviation is wholly absent, when the muscle is prevented from shortening, —while then precisely the secondary tetanus appeared particularly strong, and justified the inference of the existence of strong alterations on the surface of the muscle. These alterations (the discharges of Meissner) should therefore be distinguished from the negative deviation which follows, as a constant effect, on the compression by contraction.

The great difficulty in Meissner's theory is the negative deviation, which also the nerve-current itself gives on irritation; so far, in fact, nothing is known respecting change of form of the nerve on stimulation.

In thus provisionally adopting Meissner's view I nevertheless reserve my judgment In no investigation are so many sources of error to be met with as in that of animal electricity. We look with interest for the opinion of Du Bois-Reymond, who has laid the foundation of our knowledge in this department, and more than any one else has taught us to avoid each source of error. Though we may sometimes have to wait for his decision it is sure to come, and to be satisfactory.

[a] This explanation diminishes the value of the fact. Quite lately the fact itself (Professor Donders writes) is made very doubtful by new investigations of Heidenhain himself, under whose direction it had been found by Solger, independently of Thiry and Meyerstein.

the result of compression by contraction, which, just like any other compression, diminishes the current of the resting muscle; and, independently of this, in tetanus a series of positive discharges arises, which produce the secondary tetanus, without, in their short duration, being able wholly to counteract the negative effect of the contraction on the galvanometer.

The connexion between the phenomena here described and the contraction, with the work connected therewith, has as yet been but imperfectly explained. The cold, occurring at the moment of the contraction, would readily suggest the idea of the metamorphosis of heat into mechanical work. Against this there is, however, a decided theoretical difficulty; and we might in refutation further state, that, precisely when work is performed, namely, when the muscle is loaded, the negative thermo-electric action dependent on the cold is least—if the consecutive production of heat, which under this circumstance, also, is particularly strong, might not easily conceal the cold which has just been developed. However this may be, there is no reason for assuming that the heat produced should disappear to give place to mechanical work. The electro-motor action rather stands in causal connexion with the contraction, in so far as it determines the molecular condition which produces the contraction and the elastic tension. But our knowledge on this point is very imperfect. We do not even know whether the tension, which is perceptible on the surface of the muscles, with the work which the muscle performs by its contraction, increases or diminishes. Meissner only states that when the muscle is prevented contracting, and therefore performs no mechanical work, the negative deviation, obtained by stimulation, is much less, while nevertheless the alteration of the current ascertained by a very strong secondary tetanus, may be more powerful than when contraction ensues.

Béclard had arrived at the result that the development of heat and of dynamical work in the muscular contraction compensate each other. Of the greater chemical energy sacrificed to the contraction, when it exercises dynamical work, this work should use so much that the heat developed would be not only relatively but absolutely diminished. I have already remarked that, in dynamical action, the blood circulating freely through the muscles carries off much more heat than in statical, and that hence the less elevation of temperature in the investing skin, observed by Béclard in dynamical action, can be explained.

Whilst now Thiry and Meyerstein, in their experiments on the free muscles deprived of circulation, saw an increased production of heat in general combined with increased work, the idea of a compensation of dynamical work and of heat seems to be completely refuted.

The conclusion is this:—An electrical discharge in the muscle, the direct result of the nervous action, leads, with the development of chemical action, to a rapidly-increasing and again diminishing elastic tension (the contraction curve of Helmholtz). If a number of such discharges follow rapidly on one another, their effects combine each instant to an equal—and thus persistently to an uniform tension. In connexion with the elastic tension electro-motor action and heat arise.

The cause of the elastic tension is not known. It may be situated in the electro-motor action. This passes, however, also partly directly into heat, perhaps also into chemical energy.

It cannot be assumed that heat is changed into electric tension. Heat arises rather as the last form, partly, probably directly, from the chemical energy, partly mediately from the electrical currents and from the elastic tension remaining in the muscle after the removal of the load.

The origin of heat from elastic tension teaches that in the muscle more elastic energy is developed than is employed in mechanical work. Of the mechanical work a further part (that of the heart, stomach, intestines, &c.) passes into heat. *Consequently we recover in the mechanical work yielded externally only a part of the elastic energy produced in the muscles.*

V. RELATION BETWEEN THE PRODUCTION OF HEAT AND OF MECHANICAL WORK.

As each form of energy may be changed into heat, it is evident that each element of food may, directly or indirectly, produce heat in the body. Of mechanical work the same cannot, *a priori*, be asserted. In this case, for the possibility of origin from all elements of food, the argument is wanting, that each form of energy should be capable of giving place to mechanical work. On the contrary, we know that heat can only partially pass over into mechanical work, and that even for this conditions are required which are not present in the animal body (compare p. 8). It was thus imaginable that certain substances, on oxidation, develop only heat in all tissues, and there was, so far, nothing absurd in the supposition that non-nitrogenous bodies could produce no mechanical work, that this depends simply and solely on metamorphosis of albuminous matters. But if this theory was not absurd, it was nevertheless extremely arbitrary, and also highly improbable. Consequently, it was by no means reasonable that it should be looked upon as proved. At most, it could be looked upon only as an hypothesis. Its untenability will have appeared so soon as it shall be shown that the chemical energy of the consumed albuminous matters is not equivalent to the mechanical work performed. We propose to investigate this. To this end we shall endeavour to discover the relation between the quantities of mechanical work and of heat developed in the body, in order to be able to compare therewith those of the nutritive elements in the food used.

Traube supplies the following calculation:—[a]

A *working* horse produces 75 kilogrammeters per second, and consequently in 8 working hours 2,160,000 kilogrammeters$=$(2,160,000 : 430) 5025 calories.

A *non-working* horse consumes, according to Boussingault, in 24 hours, 2465·1 grammes of carbon and 24·4 grammes of hydrogen, affording on combustion to CO^2 and HO about $(2465{\cdot}1 \times 9{\cdot}6 + 24{\cdot}4 \times 44{\cdot}462) = 24506$ calories. Supposing that the horse, in performing work, uses only so much more organic matter as is equivalent to the work done, Traube now finds the relation between the production of heat and of work$=24506 : 5025$, that is about$=5 : 1$, and for the working time, calculated at $\frac{1}{3}$ of the day$= \frac{24506}{3} : 5025 = 8{\cdot}2 : 5$. Consequently, during the working time the mechanical work performed would amount to about $\frac{3}{5}$ of the work, represented by the heat produced.

This calculation suggests one or two remarks. In the first place, the assumption that, in work, greater consumption of matter would occur exclusively for this work, is very far from the truth. We know, in fact, that the production of heat in this case is also considerably increased, and experimentally double, nay five-fold excretion of carbonic acid has been observed. In the second place, the calculated horse-power is too great for an ordinary horse, to which the food calculated refers. We shall make a similar calculation from observations upon man, communicated, it is true, by Traube, but not employed for calculation, which will bring us to a completely different result.

Lavoisier[b] and Séguin found the consumption of oxygen by a man, not during the time of digestion, at 59° F., in the state of rest, for one hour, ...	26·66 litres,
During the performance of work, . .	63·477 „
For the increased consumption of . .	36·817 „

an amount of work was performed equivalent to the lifting of 7·343 kilogrammes to 799 metres, that is 5867 kilogrammeters$=$(5867 : 430) 13·583 calories. On the other hand, 36·817 litres$=$49·89 grammes of oxygen yield, on combustion with carbon, 179·616 ; on combustion with hydrogen even 214·905 calories. Hence it appears, that at most $\frac{1}{14}$ of the greater consumption of oxygen was used for mechanical work ; of all the oxygen consumed, not more than $\frac{1}{24}$.

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[a] Virchow's Archiv, B. xxi., page 391.

[b] Conf. Gavarret. De la chaleur produite par les êtres vivants, p. 330. Paris, 1855.

In another case, Lavoisier and Séguin found the consumption of oxygen in a man, during digestion at rest, again at 59° F. for 1 hour, . .	37·689 litres,
During the performance of work, . .	91·248 „
For the increased consumption of . .	53·559 „

a work is here performed of 6201·8 kilogrammeters=14·423 calories, while for 53·559 litres of oxygen, burned in the body, 261·274 calorics are obtained, so that of the increased consumption of oxygen, certainly not more than 14·423 : 261·274, that is $\frac{1}{18}$, and of the total consumption still less than $\frac{1}{50}$, is represented by the work performed. To these results, which would fully justify us in deriving all the mechanical work from nitrogenous combinations, we shall not attach too great importance, because we believe that the work performed in the investigations is estimated too low. But they form an admirable appendage to the calculations of Traube.

Coulomb calculates in the ascent of the Peak of Teneriffe, with De Borda, 204610 kilogrammeters per day of 8 hours, that is, 25576 per hour, or four times as much as was assumed by Lavoisier. For other work, such as the laying of paving-stones, Coulomb calculated much less, namely, 75240 kilogrammeters daily; Lamandé, 80635; Haughton, 124000 kilogrammeters, which, calculating the working time at 12 hours a day, amounts to little more than one and a half times the work found by Lavoisier. If we reckon 86000 kilogrammeters=200 calories, these are obtained by burning about 20 grammes of carbon of carbo-hydrates, while the daily consumption of carbon amounts to not less than 240. Consequently, even leaving out of view the hydrogen of the fats, &c., the work performed would be equivalent only to $\frac{1}{12}$ of the chemical energy used.—Under these circumstances, too, it was still possible to admit, that only albuminous matters produce this work.

In extraordinary exertion, under which head certainly the ascent of mountains is to be classed, the work performed is greater in reference to the heat produced. Helmholtz[a] supplies us with a calculation on this subject, based on the results obtained by Dr. E. Smith of London, who, with a compact gasometer, determined the quantity of air inspired under different circumstances (sleep and vigorous exercise not excepted), and, with a second equally portable apparatus, collected and weighed the carbonic acid expired during many hours. In the first place, Helmholtz deduces from the results obtained by Dulong and Despretz, from experiments on animals, how much the temperature of the human body ought to rise by accumulation of heat, developed in one hour, in the condition of rest, and for this he finds 2°· 16 F. This heat is the equivalent of an ascent of the body to 1,760 feet. Now, by strenuous exertion, a practised climber of mountains can attain this height in the space of one hour. But in such exertion Smith finds that the expired carbonic acid is five times greater than in the state of rest, and as only one quantity serves for the ascent itself, four times the quantity remains over for the heat produced. Hence it follows, that the equivalent of mechanical work and heat, developed in active exercise, are to one another=1 : 4. In this nothing is brought into calculation for the loss of carbonic acid by the skin, which precisely on great exertion, in horses at least, is very considerable (Gerlach). For ordinary work, the proportion of 1 : 5 is certainly estimated highly enough. The result is therefore quite different from the 3 : 5 calculated by Traube. But this does not hinder us coming so far to the same conclusion, that we think the origin of mechanical work exclusively from nitrogenous matters inconsistent with this result.

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[a] Lectures on the Conservation of Energy, delivered at the Royal Institution, April 1864.

We have here to refer to our demonstration, given in the preceding section. At the close of it (p. 17), we arrived at the result: that *in mechanical work, yielded externally, only a part of the elastic energy produced in the muscles is still found.* We remarked, that a second portion of the elastic energy—namely, what remained over in the subsequent relaxation of the muscle—must give place to heat. Evidently, however, with the elastic energy, the direct condition for mechanical work is already given. How far that will become for mechanical work depends on the loading, and is, in a certain point of view, accidental. Consumption of matter, in other words, chemical energy, is in any case no longer necessary thereto. We have, therefore, for comparison with the nutritive principles used, to know the relation between the elastic energy and the heat; and if we assume that the half of the force occurs not as mechanical work, the proportion alters from 1 : 5 to 2 : 4. If we consider, further, what numerous muscles come into action in each movement, without contributing directly to that movement, how much elastic energy in respiration, how much mechanical work in the circulation of the blood, and, moreover, in other functions, are changed into heat, we may for this also set down for $\frac{1}{6}$ part transition of elastic energy and muscular work into heat, and we then obtain for the original relation of elastic energy to all forms of work, that of 3 : 3 ; in other words, about the half of the chemical energy of our food is changed, in the muscles, into elastic energy.

Now, in ordinary feeding, we may calculate for man :—500 grammes of carbo-hydrates, to 100 grammes of fat and 100 grammes of albuminous matter. In general, these last will represent not more than $\frac{1}{5}$ of the chemical energy. If, therefore, all albuminous matters were employed for muscular work, which is by no means the case, they would be insufficient completely to explain this. It is, therefore, certain that, besides the albuminous matters, there are constituents in our food from the use of which elastic energy is produced in the muscles.

VI. NITROGENOUS PRODUCTS OF METAMORPHOSIS OF TISSUE.

In Traube's theory we see the most radical reaction upon the doctrine maintained by Liebig. If, according to the latter, mechanical work could be produced only from albuminous matters, Traube inverts the proposition, and asserts that in such work exclusively non-nitrogenous substances are used, upon the oxidation of which the nitrogenous are said to have only an indirect influence. In presence of this assertion, our task is quite different. If we had, above, to prove that not all mechanical work takes its origin from nitrogenous matters, we must now examine whether any direct share therein belongs to them. The solution appears to be attainable by determining the excreted matters. By comparative investigation of these, both qualitatively and quantitatively, in the conditions of rest and of work, it may certainly be found, what matters in the latter case are used in greater quantity. Now it is known that in work much more carbonic acid is excreted. We may further assume, that in proportion herewith the formation of water in the body will be increased. But, on the contrary, according to the investigations of Voit, the eliminated nitrogenous compounds are not essentially increased. Great weight is attached to this result. It has led Traube to apply his fermentation theory to the metamorphosis of matter in the living muscle. Now, have these investigations actually proved that in muscular work the metamorphosis of nitrogenous bodies is not increased?

As is well known Bischoff and Voit assume, that all the nitrogen disappears from the body by the kidneys and the intestinal canal—in the dog almost exclusively in the urea of the urine. In the first place, we have to inquire how far this proposition has been proved.

The question, whether nitrogen is excreted by perspiration, always presented great difficulty. If, on analysis, the nitrogen of the expired air was found to be greater than that of the inspired, it was very evident that this did not prove the fact of an absolute increase: the diminished volume of the air might equally account for it. An attempt was now first made to answer the question indirectly. If the absolute quantity of nitrogen, both in the food and drink, and in the feces and urine, were determined, the difference ascertained must represent the quantity of nitrogen which has escaped in the perspiration. Boussingault found this to be in the cow not less than $\frac{1}{6}$; in the horse more than $\frac{1}{7}$; in the turtle dove, $\frac{1}{3}$ of the nitrogen of the food. Barral, in man, found it to be about one-half; Sacc, in chickens, still more than one-half. On the

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[*] Art. Harn. Wagner's Handwörterbuch f. Physiologie.

other hand, C. G. Lehmann[a] had, under the use of highly nitrogenous diet, recovered nearly all the nitrogen of the food in the urea, while Jörgensen, in experiments continued for a month upon a sheep, found in the feces and urine 93 per cent of the nitrogen. Lastly, Bidder and Schmidt had, in some experiments upon dogs and cats, recovered nearly all the nitrogen of the food in the urea. The question now attracted the attention of Bischoff. He applied to Liebig for a volumetric method of determining the urea of the urine, and Liebig gave him, in the solution of nitrate of mercury, a fluid which answered his requirements beyond expectation. According to Voit, in fact, not only the urea is precipitated by this fluid, but also the other nitrogenous constituents of the urine, and indeed almost in the inverse ratio of their nitrogenous element; so that what the boldest imagination had not dared to expect was ascertained, the amount of nitrogen in the urine, no matter in what combination it might exist.

Meanwhile, Bischoff found at first not nearly all the nitrogen of the food in the urine; and it was therefore rather strange that he, on this ground, sought the measure of the metamorphosis of tissue in the quantity of urea excreted by the urine. The investigations of Voit, which soon after followed, appeared rather to prove this measure to be acceptable. But even in them, too, the nitrogen of the urine and feces, after correction for the altered weight of the body, always remained below that of the meat used, notwithstanding that only the minimum of the nitrogen found therein was taken into account,[a] and I therefore do not hesitate to assert, that neither did these experiments exclude the possibility, that about 10 per cent. of the urea had escaped in other ways. Haughton now finds, moreover, in man, in contrast to Barral, nearly all the nitrogen of the food in the urea and feces. Likewise Ranke, in a series of investigations which bear the best stamp; so that Barral's result is to be considered as refuted. In horned cattle, too, Henneberg found the deficit in general less than that of Boussingault; where the supply of nitrogen was small, the deficit was comparatively great—thus also there was a deficit, under circumstances in which an addition of flesh in the animal was not to be assumed. In his subsequent investigations Voit obtained, not only in four dogs anew nearly all the nitrogen of the food from the urea, but also in a pigeon which he (in order to test Boussingault's results) had fed for 124 days with peas, containing a known amount of nitrogen, the quantity of that element in the excreta being constantly determined; no deficit worth mentioning (1·1 gramme) was, after correction for a slight increase of weight, met with. Finally, an experiment upon a bull, detailed, at length, by Grouven[a], is in favour of the elimination of nearly all the nitrogen in the fæces and urine. This writer, now assumes in all further investigations and calculations, that no nitrogen escapes with the perspiration, though not, as he expressly says, because he is as fully convinced of the impossibility thereof as Bischoff and Voit, but—because otherwise the results of his investigations would not be of much use. Grouven has also convinced himself that only traces of ammonia escape, which are not taken into account: indeed, the debates upon this subject are at an end.[b]

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[a] Voit found in fresh meat, as maximum, 3·73 per cent., as minimum, 3·41, and as average, 3·59 per cent. of nitrogen, and brought into calculation only the minimum; that is, 5 per cent less than the average and 9 less than the maximum. In the calculation, too, of all subsequent experiments, he has kept to the minimum. The reason he assigns for this is, that the meat analysed was much more accurately freed from adipose tissue than that intended for food: it is, indeed, not difficult to free meat from adipose tissue to less than 1 per cent., and I believe that Voit will certainly have done this.

But is it now really certain that all the nitrogen is excreted by the kidneys and the intestinal canal? that not a trace of nitrogen disappears through the lungs or skin?

To the results of indirect experimentation above detailed those of direct investigation are strictly opposed. I allude to those of the investigations of Regnault and Reiset, continued for years by Reiset, and engaged in on a smaller scale also by Marchand. Regnault and Reiset caused animals to breathe in a closed space for about 24 hours, while, by a peculiar mechanism, the carbonic acid formed was constantly absorbed, and was replaced by an equal volume of oxygen. Where they found, at the end of the experiment, the absolute quantity of nitrogen in the enclosed space increased, they inferred that nitrogen was excreted by perspiration. Voit made it appear, as if Regnault and Reiset found the nitrogen at one time increased, at another diminished, without any assignable cause, or any regularity. In this he is deceived. The experiments show that, in warm-blooded animals, the quantity of expired nitrogen is usually less than $\frac{1}{100}$, often amounts to only $\frac{1}{200}$, and never exceeds $\frac{1}{50}$ of the oxygen consumed. But only under particular circumstances, namely, after the withholding of food, almost without exception in birds, sometimes also in mammalia (in marmots during hybernation), was the nitrogen, in place of being increased, found to be diminished. Reiset continued these investigations for years, and his results communicated last year to the Institute, of experiments upon sheep, calves, and wild boars, are similar to the former conclusions. Marchand, too, found for 100 volumes of carbonic acid 0·94 volumes of nitrogen expired.

All these investigations were blotted out by Voit with a stroke of his pen. We may regret with him that Regnault and Reiset appear to have neglected to control the closing of their apparatus. But has he the right, therefore, to set aside these results without having repeated the experiments in a controlled and correct apparatus, with a negative result? With such experiments he would much better have silenced his adversaries, than with a philippic on their obstinacy. It is evident that in the controversy facts are arrayed against facts. We shall not defend the results obtained by earlier investigators against those that are more recent. We acknowledge that in the force of some results of indirect experimentations there is little to object to. According to the experiments so long continued by Voit upon one and the same pigeon, we must believe that a pigeon can live without losing nitrogen, otherwise than by the feces and urine. But all the other experiments, even those of the more modern observers—leaving out of consideration the frequent deficit of nitrogen—admit the possibility, partly on account of the short duration of the experiments and the impossibility of bringing into account with perfect accuracy the change of bodily weight—partly on account of the uncertain amount (probably calculated too low) of nitrogen in the meat used—partly (in horned cattle) on account of the very variable contents of the intestine (Grouven)—that some nitrogen, as such, disappears through the perspiration. These experiments can, consequently, not refute the existence of this excretion; and with our eye upon the results of the direct experimentation, we are not at liberty flatly to deny it.

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[a] Physiologisch-chemische Fütterungs-Versuche. Berlin, 1864, p. 121.

[b] Conf. Kühne and Strauch. Centralblatt f. d. mediz. Wissenschaften, 1864, Nos. 36, 37.

VII. INFLUENCE OF MUSCULAR WORK UPON THE PRODUCTS OF METAMORPHOSIS OF MATTER. OBSERVATIONS OF VERLOREN AND OF VOIT.

In the first place, I may here communicate the observations of Dr. M. C. Verloren. They were made and communicated in our circle, "amicitia naturæ interpres," at least two years before the investigations of Voit were known, and before Traube established thereon his theory. On my requesting that he would be good enough to give me, in writing, the leading points of his statement, I had the satisfaction to receive the following answer, which will certainly be interesting to my readers:—

"The observations upon insects, which had already led me to the conviction that muscular work is attended with no considerable metamorphosis of albuminous matter, consist in the following:—

"1°. Many insects use, during a period in which very little muscular work is performed, food containing chiefly albuminous matter; on the contrary, at a time when the muscular work is very considerable, they live exclusively, or almost exclusively, on food free from nitrogen.

"2°. During the first period a very great quantity of urine is excreted, in proportion to the respiration, and the respiration also is absolutely little; during the second period, on the contrary, the urinary secretion is slight in proportion to the respiration, and the respiration also is very considerable.

"Of the first proposition I formerly quoted bees as an example, and they have since been indicated also by others. But as I do not know whether this argument has been brought forward in its full force I shall say something on the subject.

"Bees have three sorts of cells: 1°, for working bees; 2°, for male bees; 3°, for queens. The first are by far the most numerous, the smallest, and are of the same form and capacity as those in which the honey is collected and kept; the second are somewhat larger; and the third are still larger and of a totally different shape. In the bottom of each of these cells the queen lays an egg. After some days a worm-like larva emerges from the latter, lying on the bottom of the cell. The working bees then daily bring these larvæ food, which they pour out into the cells. This the larvæ appropriate, become larger, and at last fill nearly the whole cell; they are then full-grown, and spin a dense web, closing the openings of the cells from above. They then no longer receive any food, but undergo this change, whereby a completely new being is formed out of the accumulated food, with totally different organs and tissues. When the bee is fully formed, it gnaws away the spun covering, and comes out. It now enters upon its active period of life, in which, during a considerable time, it performs enormous muscular labour in its numerous and very rapid flights, in its great industry and vivacity; bees are, in fact, the most restless and most lively of insects. In the larva state, on the contrary, their muscular work is as slight as possible, being confined to swallowing the food, turning occasionally in the cell, and spinning the closing material of the same.

"Now, of what does the food of bees consist? Of the so-called *bee-bread*, that is pollen from the flowers, with a little honey; and, 2ndly, of honey. The first, containing a large quantity of albuminous matters is exclusively the food of the larvæ; the second of the full-grown bees, which use very great quantities thereof, as is evident from the considerable amount of it which they accumulate, to serve as a store during the Winter, and when from other circumstances, they cannot go to fetch it. In the Winter there are no larvæ.

"I have already referred to the butterflies. The caterpillars, it is true, develop more muscular work than the larvæ of bees; they must themselves, crawling, seek their food, must spin more, or crawl more, to seek for a suitable place for transformation to the chrysalis state. Still this is only inconsiderable when compared with the action of the restless flying butterflies. Now, the caterpillar feeds on leaves containing albuminous matter; but the flying butterfly seeks only honey from the flowers; their mouths are even not adapted for the reception of any other food.

"I now pass over to the second point, from which alone the first derives its full force, and to which my investigations have more decided reference.

"During the larva condition there has been formed from the food consumed, in addition to the organs of the caterpillar or larva, a thick layer of adipose tissue, consisting of albuminous matter and fat, situated beneath the skin with its muscles, and surrounding the intestines. In the chrysalis state this adipose tissue again becomes fluid, and is taken up into the blood, as are likewise the existing organs of the caterpillar, and out of these materials totally new organs and tissues are formed, producing an entirely new being, the perfect insect. During this transformation no more food is taken in, but what is already present in the animal is quite unmixed and ample. Neither are any excrementitious matter formed and voided ; the respiration alone continues, so that the chrysalis constantly decreases in weight. In this metamorphosis of the organs, and in the course of the other vital functions of the insect, urine is, at the same time, in connexion with the respiration, secreted ; this, however, is not evacuated, but collects in a great vesicular process, which arises at the rectum, in the formation of the organs of the perfect insect. Immediately upon, and shortly after leaving the chrysalis envelope, the perfect insect expels the whole quantity of urine formed and collected during its chrysalis existence.

"I must also make mention of the prolongation beyond a year of the chrysalis state, a phenomenon which takes place in some insects. Most insects are in their different states bound to definite periods of the year. Now, it happens in some insects, that when the time for the chrysales to emerge is past, some have not come out ; these, then, lie over for another year, in order then to emerge at the same time as those which did not pass the year. In *Sphinx Ligustri* this often takes place. It is to that insect that the following results refer. It remains about 300 days in the chrysalis state, so that those which pass the year, continue in it for 665 days. The investigations were made on a certain number of insects, varying for the different investigations, but separate for each individual. The result here given is the average of the several observations.

"The following chrysales have thus, during the chrysalis state, given, in urinary secretion and respiratory loss, calculated in per centage of the weight of the chrysales on the 66th day before the emergence of the butterfly, as follows :—

		Urinary Secretion.	
Annual,	♂ ♂	45·89	per cent.
,,	♀ ♀	31·76	,,
Biennial,	♀ ♀	35·64	,,
Further urinary secretion for the additional year,		3·88	,,

	Respiratory loss.	
Annual, . .	10·71 per cent.	
" . .	10·43 "	
Biennial, . .	10·40 "	respiratory loss in 300 days.
Further urinary secretion for the additional year .	7.74 "	respiratory loss in the additional year,

"The relation of the urinary secretion to respiratory loss in females for one year, and for the extra year in the biennial females, is therefore :—

For 100 urinary secretion, 32·75 respiratory loss in those of one year.
" " " " 199·48 " " for the extra year.

"The butterflies have yielded in urinary secretion and respiration, per hour, in milligrammes :—

	♂	♀
Urinary secretion,	0·326	0·425
Respiration,	1·253	1·252

"For the butterflies I cannot easily state the quantities in per centage upon the bodily weight, but for the relation between the urinary secretion and respiration, which is here principally in question, this is not necessary. We see that this is about as follows :—

Chrysales.			Butterflies.		
Annual, ♂	Annual, ♀	Biennial, ♀ Extra year.	♀	♂	
4	3	½	⅓	¼	Urinary secretion to respiration,

so that in the male chrysalis fifteen times more urine is excreted than in the male butterfly.

"The greater urinary secretion in the male chrysales than in the female, is constant. Now, the chrysalis performs scarcely any muscular work; the butterfly an enormous amount."

"When we take into account that the ♂ chrysalis excretes nearly half its weight in urine, when we add thereto what is lost in respiration in the chrysalis envelope and in the exhalation of the butterfly on emerging, which is very considerable; when we, furthermore, reflect that the butterfly consists only in small part of muscles, and that it takes in *no more albumen whatever* ; it cannot be otherwise than that for muscular work but very little albumen is metamorphosed."

"As the butterfly does not weigh the half of the chrysalis, and nearly the half of the chrysalis passes into the urinary secretion, the whole butterfly would necessarily be wasted away if it would consume as much albumen as the chrysalis. But the muscles and the albumen still present, which might be used for muscular work, will not amount to a fourth part of the butterfly. If, therefore, the butterfly entirely consumed its muscles, it would have used only a fourth of the amount of albumen used by the chrysalis. But muscles, which are no more cannot perform any function, so that only a very small portion of the muscle can be consumed if it is still to remain in a serviceable state. I will assume a tenth, in which case the butterfly has at its disposal only a fortieth part of the quantity of albumen consumed by the chrysalis. And yet the butterfly performs muscular work, and the chrysalis almost none.

"It seems to me that it is as clear as possible that the albuminous matters exclusively, or at least almost exclusively, are metamorphosed in the formation of the tissues and organs, and that the non-nitrogenous nutritive matters, by their disintegration, produce work.

"I will yet remark, that the observations on the butterflies deserve the least confidence, but that in more accurate essays they would tell still more to my advantage, that probably the respiration will prove to be still greater, and the urinary secretion as much less. Also that the first three respiration numbers in the chrysales must really be somewhat higher, but too little so to have any influence upon the results."

In heartily thanking my friend Verloren for the communication of his investigations, which are as important as they are accurate, he will not take it ill of me if I bring under the eye of our readers my reflections, which are well known to him, against the inference he has drawn from them. That the larvæ, in their development, that is, in the building up of albumen-containing organs, take in albuminous matters in their food, is not to be considered strange. Out of albuminous matters the young bird in the egg is also principally developed; and the embryo of the mammal, which, except the movements of the heart, certainly performs very little work, needs for its development also chiefly albuminous matters. When the larva is full-grown, a period of quite peculiar metamorphosis ensues. That then, while from the old a new being is formed, many albuminous matters should be metamorphosed, cannot surprise us. The phenomenon simply proves that metamorphosis of albuminous matter exists without any connexion with movement, and on this point there is no room for doubt. From the experiments of C. G. Lehmann and Hasse we have already learned, that feeding with highly albuminous matters, independently of muscular work, considerably increases the excretion of urea, and in the excreted urea we can consequently find no measure of work performed. Further, it appears inadmissible to compare the consumption of matter of the perfect insect with that of the chrysalis state, with which such peculiar nutritive changes are bound up. That consumption of matter must be considered by itself; and then the question is only, how much muscular work the insects perform, and how much nitrogenous and non-nitrogenous matter is at the same time metamorphosed. And as to this work: by the remark, that the animals are quick and lively, and perform long journeys, it is not measured, as little as all nitrogenous products of the metamorphosis of matter are determined by the excreted uric acid. Lastly, I am somewhat sceptical as to the composition of the food taken in. Does the butterfly suck so exclusively non-nitrogenous honey out of the flowers? Precisely in Winter, when the bees do not stir, this non-nitrogenous honey appears to be sufficient. Undoubtedly, the facts collected by Verloren are important; they show, that in insects much albuminous matter is used, and much uric acid formed, without the intervention of movement. They shed light upon the metamorphosis of matter connected with the several stages of insect life. But to the question, how much nitrogenous matter is consumed in reference to the equivalent mechanical work they give no satisfactory answer.

We now come to the investigations of Voit. They are confined to the determination (by Liebig's volumetric fluid) of the amount of urea contained in the urine of a dog both during abstinence and during regulated feeding with meat, in both cases with and without the performance of mechanical work.

The work which the dog had to perform consisted in running in a tread-mill. Voit calculates this (indeed somewhat liberally) at 150,000 kilogrammeters daily. The observations extend over four series; 1°, one of five days, during the withholding of food; on the second and fourth day with work, 16·6 grammes of urea were excreted; on the three other days, in a state of rest, the amount was 14·3 grammes. 2°, one of nine days, food being likewise withheld, on the fourth, fifth, and sixth days, with work, 12·33 grammes were excreted; on the other days, the average amount was 10·88 grammes. 3° and 4°, two series, on each of three days' work, followed by three days of rest, begun after an equilibrium of ingesta and egesta had been attained by regular feeding with meat; on the days of rest the excretion amounted on an average to from 109 to 110 grammes; on those of work to 114, when the labour was performed before eating (3rd series); to 117, when it was performed after eating (4th series).

These are briefly the facts. They lead to the conclusion that during work the metamorphosis of nitrogenous matter in the muscles is not increased. Theoretical considerations connected therewith, to which Voit himself will certainly no longer attach much value, we leave out of the question. It is with the direct conclusion we have to do, and against this we have some difficulties to bring forward.

In the first place we observe, that on the days of work actually somewhat more urea was found, which Voit, without sufficient proof, thinks is to be ascribed to the increased excretion of urine. A second difficulty relates to the method followed. Does the volumetric fluid really show the amount of nitrogen present? Besides urea there are several nitrogenous matters in the urine; and Voit thinks he has satisfied himself that the amount of nitrogen in these also is approximatively shown by the volumetric fluid—so soon as one or other of these matters, under the extraordinary circumstances of violent muscular work, is disproportionately increased, we can certainly no longer rely upon the method. Now, to deny, *à priori*, the possibility of such an increase, because even in the state of rest also a number of muscles are active,[a] appears to me presumptuous; and, looking to the results of experiments obtained on other animals with respect to hippuric acid, inadmissible. I, therefore, with Meissner, think it desirable, in such experiments, to determine the amount of nitrogen directly. Our third difficulty is, that Voit feels himself justified in neglecting the loss of urea by the skin, and in denying the loss of nitrogen by perspiration. How great the amount of urea of the sweat in work may be, we know not. But in a dog, which wearies itself in the highest degree by work, it may come into consideration. Remarkably enough, Speck found in a man, that the urea of the urine was not increased only when the work was accompanied by copious perspiration. And as to the escape of nitrogen in the perspiration, we have in the preceding section shown that the results of the direct investigation are not satisfactorily refuted by those of the mediate. Now, if there is such a loss in the state of rest, there is every reason to assume that in muscular work it will be considerably increased. Even for these reasons we think that the conclusion, that in muscular work the metamorphosis of nitrogenous matter is not increased, is liable to doubt. But there is more.

What is the origin of the urea, determined by Voit. Is it wholly in the muscles? Certainly not. For the greatest part, at least? This, too, is improbable. In the nutritive fluid of muscles we find creatin, creatinin, and inosinic acid, no urea; and how far the substances mentioned are changed into urea, has not been ascertained. In every case, too, creatin and creatinin occur in the urine; and no one has shown that these should not be met with in greater quantity in muscular work. Evidently, therefore, the metamorphosis of nitrogenous matter in the muscles might be considerably increased without much of it being discovered by the volumetric method applied to the urine. It should not be forgotten that, independently of muscular work, much urea is produced—the more so, in proportion as more albuminous matter is supplied to the body; and in order to establish the increased consumption in the muscles, it is therefore certainly not enough to compare the ascertained increase of nitrogenous products of the metamorphosis of matter with the sum total of these products. It should further be borne in mind, that a greater metamorphosis during work, is followed by a slighter change during rest (Speck), and that this subsequent diminution might conceal the greater consumption of nitrogenous matter during work.

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[a] Conf. Voit, in Liebig's Annalen. 1863. II. Supplement-Band, p. 376.

Lastly, we have to refer to a singular result, to which Voit's theory would lead us. It is proved that the production of carbonic acid increases very considerably in muscular work. In a good walk, in which from two to three English miles (three miles are nearly an hour's walk) are performed in the hour, Smith found the quantity of carbonic acid expired from 1⅘ to 2⅖ times greater than in the state of rest; in ascending a mountain it was five times greater; and when seven miles were accomplished in the hour the quantity of carbonic acid was multiplied seven-fold. Now, supposing that a dog, on pure meat diet, daily performs work, by what other matters can the increased quantity of carbonic acid be permanently supplied if not by the meat consumed? And if the nitrogen were not at the same time excreted, and indeed in the same proportion, should we not finally have the prodigy of a dog existing on pure nitrogen? This is what is called an *argumentum ex absurdo.* It may at least prove that experiments such as those of Voit, in order to lead to certain results, must be continued much longer on the same animal.

VIII. METAMORPHOSIS OF MATTER IN THE MUSCLES.

Not only the excreted products of the metamorphosis of matter, of which we have been speaking, but also the products in the muscles themselves may throw light upon the matters consumed in work. That the consumption during work is more active than in the state of rest is easily proved. In the first place, it is already included in the mechanical work produced together with increased warmth in the muscles themselves, which can have no other origin than than the chemical energy of the matters consumed therein. But, moreover, it appears directly in the examination of the outflowing blood. Already Bernard had observed, that in contraction of the muscles this blood is particularly dark; and Ludwig and Sczelkow[a] once found only 1·3 per cent. of oxygen remaining therein—only about the third part of the average quantity in the state of rest. Remarkably enough, the volume of carbonic acid obtained was sometimes greater than that of the oxygen which had disappeared. The question now is, what matters were here used? If the proportion of oxygen which has disappeared, and of the carbonic acid obtained, just mentioned, be established, we should be readily inclined to place the oxidation of carbo-hydrates quite in the foreground. But even then the hypothesis would still be required that a part also of the oxygen of the carbo-hydrates should have served in the production of carbonic acid, with the simultaneous development of a compound abounding in hydrogen.

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[a] Sitzungsberichte der Wiener Academie. 1862, B. xlv., p. 171.

But we should hesitate to take refuge in such a supposition, although Pettenkofer and Voit, as is well known, in the calculation of excreted carbonic acid and absorbed oxygen (the latter, however, having been very indirectly determined) in dogs, fed partly with sugar or starch, arrived at the same unexpected result. But we certainly admit that sugar is consumed in the muscles. A small quantity of sugar actually exists in the muscles (Meissner), and sugar is also constantly conveyed with the blood. The same is true of the fats. The muscular tissue itself contains no inconsiderable amount thereof, and the blood incessantly supplies fats. It is true that it is conceivable that both fats and carbo-hydrates may be formed in the muscles themselves, from albuminous matters (Meissner assumes this of the sugar found to exist there); but with the great quantities of mechanical work and heat, which in fatigue of the whole body, may be supplied by some few muscles, without alternation with rest, the necessity of the supply of matters from the blood, during working, is to be admitted. In the never-resting heart it can even not be otherwise.

Neither has the examination of the nutritive fluid of the muscles been neglected. Helmholtz began with a comparative determination of the extractive matters, and found after contraction those soluble in alcohol increased, those soluble in water diminished. The lactic acid of the muscles has become important, especially from the investigations of du Bois-Reymond, who proved that it is formed in greater quantity in contraction, so that even during life an acid reaction may predominate just as occurs in all muscles after death, when the nascent lactic acid is no longer neutralised and carried away by the blood. From what matters the lactic acid is formed in the muscle is not shown. Perhaps the fact that in dogs the acid reaction during life occurs with great difficulty, while in rabbits it takes place very readily, may be brought into connexion with the difference of the alimentary principles used.—It is further known that here, too, nitrogenous products of metamorphosis of matter are developed, among which creatin and creatinin occupy a prominent place. Bearing in view Traube's theory, it was certainly an important question, whether these are produced in greater quantity during the action of the muscles. The investigation carried on by Sarakov, under the guidance of Kühne, has now answered this question in the affirmative; acid muscles are found to be richer in creatinin than alkaline muscles are; and after tetanisation the amount of creatin and creatinin was sometimes more than doubled.[a] Even many years ago, Liebig and Gregory found[b] in the flesh of a fox, which had been for 200 days well fed in his kennel, and had become fat, not the tenth part of the creatin met with in the flesh of one shot in the chase.

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[a] Virchow's Archiv, xxviii, p. 544.

[b] Ann. der Chemie und Pharmacie, B. 74, p. 100.

The inference to be deduced from all this is evident: nitrogenous matters, too, are, during the action of muscles, metamorphosed in increased quantity.

In the metamorphosis of matters lies the cause of the fatigue of the muscle. The principle agent herein is certainly the accumulation of the products of the metamorphosis of matter in the organ. We have above (p. 13) already shown, that tonic tension of a muscle, without any external work, very rapidly produces fatigue, in consequence of the impeded removal of the products of metamorphosis of matter. The investigations of Ranke[a] fully corroborate this. They show that the removal of these matters is sufficient to remove fatigue, while injection of these matters (the extract of fatigued muscles, or even of lactic acid alone) suffices forthwith to elicit the phenomena peculiar to weariness, which then again give way on subsequent neutralization. The significance of the acid reaction for fatigue is confirmed also by the fact that in the never-wearied heart this reaction is absent during the whole of life—without doubt in consequence of conditions favourable to removal. But, on the other hand, we must not overlook the truth, that the supply of matters to the muscle is also a condition of its action. The fact that fatigue after work is more slowly recovered from than that after simple tonic tension, though also continued to total inability, may very well be connected with the need of new matters. In fact, it seems not possible that the removal of the existing products should require so much time as that during which the fatigue lasts. It is certain that the muscle has by continued action become in many respects another organ, in which the metamorphosis also is not yet equal to that occurring in a state of rest. This change of the organ, the altered composition especially of the nutrient fluid, must have a many-sided influence on the interchange of its constituents with those of the blood. That on the occurrence of the acid reaction the supply of albuminous matters will be limited, is in an ingenious manner brought by Heynsius into connexion with the periodicity of many vital phenomena.

IX. APPLICATION OF THE THEORY.

For practical application the theory is of little importance. We must be modest, and acknowledge that respecting the influence of muscular work on the metamorphosis of albuminous matters, we do not know much more than might in all strictness have been assumed since the investigations of C. G. Lehmann. By these it had already been shown, that with the use of a great quantity of albuminous matters the urea of the urine is considerably increased, and that in muscular work this increase (from 32 to 36 and 37·4 grammes) is comparatively slight. Others came to the same conclusion: in the first place, Draper, who, on this subject, even thought it necessary to resort to the excretion of nitrogen in other ways; moreover, L. Lehmann, who met with increase only in a boy, and in a slight degree in women, not in men. In Speek's experiments the increase was somewhat more considerable. But in all we arrive at the result that, if only the ascertained excretion of urea be taken into account, in comparison with the augmented production of carbonic acid, the metamorphosis of non-nitrogenous matters in muscular work increases in a much greater degree than that of the nitrogenous. We readily admit this. We believe even that, as all excretion of nitrogen might have been taken into account, at least in experiments extending over only a few days, the augmented consumption would still have been found to be greater in the non-nitrogenous matters: for these few days of work fat of the body may be consumed.

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[a] Archiv f. Anatomie, &c. By Reichert and du Bois-Reymond. Jahrg., 1863, p. 422.

But all these results bring us not a step further. They show only that non-nitrogenous and nitrogenous matters are both necessary for muscular work, as they are for life in general.

Two extremes were maintained, which appeared to be in a position to neutralise each other.

A few years ago Bischoff and Voit still held to Liebig's theory:—"It is and must continue true in all ages, that only these nitrogenous substances are the creators of force, that is, that they alone, by their metamorphosis in the animal body, give rise to effects of power, to motor phenomena. And in like manner it will remain incontrovertible, that the fat and the so-called carbo-hydrates, by their metamorphosis, produce only heat and no motor effects." And soon after Traube comes forward and asserts, with equal over-confidence:—"The organized part of the muscle is not destroyed in its work." "The metamorphosis of matter in the organized part of the muscle is neither connected with its action, nor is it increased by the latter." "In muscular action albuminous bodies in general are not destroyed." I would refer especially to the positiveness of these last words. It is remarkable that Traube erects his diametrically-opposite theory upon the results obtained by the experiments of Bischoff and Voit. The latter have now indeed given up their exclusiveness, and we soon find Traube also vacillating:[a]—"My theory does not, indeed, require," he says in a subdued tone, "that exclusively non-nitrogenous substances should be employed in the maintenance of muscular action, but it requires that the excretion of urea should not increase in proportion to the muscular activity." To such a modest postulate we can have no objection. To it not a single fact is opposed. It answers any mode of feeding; for, as the metamorphosis in the body never increases in so great proportion as the muscular work, even in continued exclusively animal diet, all the nitrogenous products cannot increase at all in proportion to the work performed. But I repeat, on practical ground this brings our knowledge not one step further. It does not teach us with what relation between nitrogenous and non-nitrogenous alimentary principles an animal or a man is best adapted for muscular work, and this was the problem we had to solve.

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[a] Virchow's Archiv, B. xxiii., p. 196.

Of one thing we may be convinced. What we give in the food, and what maintains the body in a comparatively healthy state, will also be used in muscular work. If we give more albumen, the metamorphosis of albumen increases: if we give more fats and carbo-hydrates, the consumption of these in the muscles must also increase. In the greatest differences in diet, in fact, so much work is performed, that in the one case the nitrogenous, in the other the non-nitrogenous matters of the food taken in are undoubtedly insufficient to explain all the work, and they must consequently in muscular work be capable of in part replacing each other. The question is not at present how far the nitrogenous matters first yield non-nitrogenous, which are then further oxidised; the fact is that the nitrogenous are consumed, and, as the increased amount of creatin in muscular work proves, in the muscle itself. It is, indeed, a well-ascertained fact, that under the most varied proportions of the alimentary principles in the food consumed, some men accomplish violent labour, others indulge in a life of ease. In the former as well as in the latter, these proportions admit of a wide range. The system accommodates itself. We were already aware, and fresh experiments of Voit have more precisely proved and explained, that the body cuts its coat according to its cloth. If much food be given it, it uses much; with a scanty supply it observes a wise frugality. This holds good of food in general, and it is applicable also to each of the alimentary principles. If to a regularly-fed dog we diminish the supply of albuminous matter, the supply is at first exceeded by the consumption; but the diminution of albuminous matters in the body, which is the consequence hereof, gradually lessens also the metamorphosis, and thus the equilibrium between supply and waste is quickly restored. On the other hand, with a more abundant supply of albuminous substance, more of the latter remains in the body, and the consequently increased metamorphosis in this case also restores the equilibrium between supply and consumption. These results, obtained by Voit in the dog, were confirmed by Ranke in the human subject. He obtained, so far as the excretion by urine and feces could show it, an equilibrium in two series of experiments, once with a proportion of nitrogen to carbon$=1:11$, and again with the proportion of $1:15$.

Now, what proportion is the most suitable to man for the discharge of his functions in general, as well as for the production of muscular work.

With this question we turn to direct experience. From it alone an answer is to be expected:—That in different animals, or even in different men, according to individual peculiarity, another proportion will be required, is evident, *a priori.* We are therefore to look for no definite conclusion, where only a general, though satisfactory indication can be given. As a transition to direct experience, we have first to point to a remarkable result of the experiments of Bischoff and Voit,[a] who proved that in alteration of diet the amount of water in the body undergoes a modification. When a dog, after having for some time been fed with vegetable matters, got a liberal supply of meat, the water was removed from his body in streams: by the urine alone, 120 grammes (about four ounces) more were excreted than was taken in with all the food and drink together. In accordance herewith the muscles after the use of animal food were richer in solid constituents than after feeding with vegetable matters. Hence it follows, in general, that the accommodation of the animal to a certain food depends on an essential change in its body, and we attach especial value to the particular fact that animal food makes the muscles firmer, richer in solid substance, poorer in water. It might be alleged against the force of these experiments that they were performed upon carnivorous animals—the dog and the cat—that these digested vegetable food less perfectly, and that the increased amount of water in their muscles, when upon vegetable food, is to be looked upon as morbid. But this objection, the value of which we shall not here more closely investigate, in no case affects the experiments of J. Ranke,[b] which led in man to a similar result. These experiments are, in fact, extremely important. It appeared that man, in proportion to his bodily weight, cannot digest nearly so much meat as the dog. This quantity was, moreover, insufficient to support the body. Under the most liberal use possible, during which digestive disturbances were not wholly absent, more carbonic acid was removed by the perspiration than the carbon of the meat consumed could yield. Consequently, some of the fat of the body must have been used, and at the same time the deficiency of nitrogen in the urine and feces was so great that the experimenter was obliged to infer that albuminous matters were deposited in the body. Under these circumstances much more water was removed than was taken in. The change effected by the increased supply of albuminous matters is consequently this, that the body becomes richer in such matters, and at the same time poorer in fat and in water.

In this, too, we recognise a certain accommodation. By the excessive use of meat the human organism evidently approaches more and more to that of the carnivorous animal, and we believe, with Ranke, that in very lean, but at the same time healthy men, the metamorphosis of matter agrees more closely with that of carnivorous animals.

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[a] Die Gesetze der Ernährung des Fleischfressers. 1860. [The laws of the nutrition of the carnivora.]

[b] Archiv f. Anat., &c., 1862, p 311.

X. THE FEEDING OF WORKING ANIMALS.

In order to prove that vegetable food is better adapted for muscular work than animal food is, an appeal has been made to our beasts of burden. These belong, in fact, almost without exception, to the herbivora. But the argument, borrowed from them, has, however, no great value. It is much as if we should infer, from the use of turf and coal for warming, that fats should not be adapted for fuel. Need it be said that we choose the herbivora, in the first place, as beasts of burden, because they are more cheaply kept? In some places people came to the resolution to confine themselves in the zoological garden to herbivorous animals, not in order to make them work, but simply for economy. Other reasons might, undoubtedly, still be adduced why we do not by preference yoke lions and tigers to our carriages.

That carnivorous animals are not otherwise unavailable as beasts of burden is proved by the case of dogs. In the high latitudes of Asia and America, where they are more equally kept than any other animals, they are, as Parry informs us, used for the conveyance of burdens, and they perform, six or eight being yoked to one sledge, day-journeys of from 40 to 50 English miles, with a load of from 800 to 1,000 pounds weight.[a] And with us, too, dogs worked in the country in the treading wheel, in order to churn; in town, under milk and vegetable carts; and occasionally on the high roads, under dog-cars, which, heavily laden, were drawn along, driver and all, sometimes by a couple of dogs, with incredible rapidity. It is known that these dogs work best when, with bread and milk, they are fed on the refuse of meat and bones.

But if, however, herbivorous animals are more advantageous as beasts of burden, the largest in general are to be preferred. Among these are our horses, oxen, asses, and mules; also buffaloes, camels, and dromedaries. Large animals are more suitable for most work, not only because a single one can move a large load, but they are also more advantageous, because, with a smaller surface in proportion to their mass, they lose comparatively less heat, and the consumption of matter can consequently be proportionally more useful in the production of mechanical work.[b] In this point of view the elephant would be the best beast of burden; but for most work he presents practical difficulties, and since the introduction of firearms he has been disused in war.

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[a] These and other particulars I have taken from an address by Bruecke: Die Arbeitsthiere. Wien, 1854. Delivered on the 30th May of that year, at the commemorative sitting of the Imperial Academy.

[b] Conf. De stofwisseling als bron der warmte van planten en dieren. [The metamorphosis of matter as the source of heat in plants and animals.] Utrecht, 1845; as well as Bergmann, Thierische Wärme [Animal Heat] in Wagner's Handwörterbuch.

Now, is it a matter of indifference what food these animals use? Experience has taught the contrary. Highly-bred horses need highly-nitrogenous food. Their excellent qualities are developed under the influence of such aliments, combined with exercise and care for perfect transpiration. Thus, in these horses the cubic centimetre of muscle has reached a higher labour value than in any other working animal. In order not to degenerate they now require such food and require it permanently. The Arab never lets his horse eat grass and straw to satiety. His chief food is barley, and in the wilderness he gets milk, and if great effort be required, even camel's flesh. The horses, which in the Sahara, are used for hunting ostriches, live nearly exclusively on camel's milk and dried beans. Of our horses, too, it is well known that in order to do heavy work they require more than grass and hay. Oats are necessary to give strength and activity; and while running the food preferred for the horses is bread. I have consulted many innkeepers and coachmen upon the feeding of horses. Their verdict has been unanimously as follows:—"The oats must be in them. If they come from the farmer they are round and plump: the farmers feed well. But such horses are not fit for our use. They sweat directly. They cannot bear a long run. If they have had hay the whole time, although they then get plenty of bread and oats for a couple of days, they cannot run upon it. The oats must be in them; they then need not eat much by the way. To feed well constantly, that is the point. Hard running depends upon the breathing. If they have stopped for a time, they must again accustom themselves to running; but in a few days they are ready, provided they have all through had plenty of oats. Grass is not much, but it is good for the health." In these simple words of the man whose eagerness for gain had sharpened his faculties of observation, lies a nucleus of truth. In our language it signifies that horses, in order to perform hard work, must have not watery, but firm muscles. And the food which serves best to produce such muscles is then further necessary to keep them in this condition. It is remarkable that reference is so generally made to the tendency to sweating in a horse not very well fed. Perhaps this is connected with the greater amount of water in the thick rotund body. But certainly in perfect muscles the proportion of work produced to heat is also more favourable. In this, too, exercise bears its part. If albuminous matters yield dry muscles, exercise makes them red. My friend Dr. Luijten[a] showed, many years ago, that the muscles have more colour the more they are used, and that their redness depends principally upon the blood contained in the vessels. One of the results of exercise will therefore be a better circulation in the muscles, on which not only the supply of oxygen and fuel (*sit venia verbo*), but also the removal of the products of the metamorphosis of matter depends. Strikingly correct, too, is the expression, "hard running depends upon the breathing." If the Arabian horses are the most valuable, among the English race we find the fleetest racers. They move with the rapidity of a violent hurricane (25 metres per second), and for a short time leave even the express trains of England behind them. To this, in the first place, a peculiar structure of the limbs contribute; but it depends chiefly upon the lungs, which have in a comparatively short time to exchange the oxygen for the carbonic acid formed. We feel this in ourselves. In a rapid movement, especially in rapidly ascending a hill, our breath in a few minutes becomes too short, before any fatigue worth speaking of is felt in the limbs. Horses which are accustomed daily to do a certain work, step by step, are not in a state to do the same work trotting in fewer hours of the day.

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[a] De musculorum rubore. Diss. inaug. Trajecti ad Rhenum, 1840.

The conclusion is this, that the best and noblest races are developed and maintained upon a diet abounding in albumen, and that each horse is better adapted for work when fed upon oats, bread, and beans, than upon hay and grass.

The labour of oxen is far behind that of horses; these animals are, however, still used here and there, in the Netherlands, too, among others, in the establishments of Veenhuizen and the Ommerschans. In general they live on grass and hay; but if they have hard work to perform they get, with a more liberal allowance of their ordinary food, so-called oxen-bread, baked principally of bean-meal, and conseqently rich in albuminous matters. Without this bread they fail in this work.

What we read of the reindeer is important. Brooke tells us that in one day it not unfrequently runs for nineteen hours, performing 150 English miles, yoked to a sledge of 240 pounds. This work is equal to that of four Esquimaux dogs. Now in the Imperial Menagerie at Schönbrunn a reindeer gets as his daily food 2·24 kilogrammes (nearly five pounds avoirdupois) of Iceland moss. But Brücke tells us that the food of the rein-deer is not always so sparing, that in Summer he seeks the mountain pastures, and in Winter lives on the parasitic plants of the forests, and for want of other nitrogenous food catches and devours field mice.

Moreover, the accounts respecting the long journeys which camels take, laden with heavy burdens, border on the incredible. And yet it is a question whether in these journeys they perform very much work. In estimating this the chief element is, how much the animals, in moving, raise their bodies, and with them the load which they carry. I am not aware that any observations have been made on this point, but I venture to suggest that camels are indebted for their extraordinary capability of carrying loads partly to the slight displacement undergone by the latter in a vertical direction. Of camels we know that in their large anterior stomach they can take in at once an abundant supply of food and water, and that, moreover, they have a store in their humps, the fat of which, in the absence of food, is consumed on long journeys. In other respects I do not find sufficient information on record, as to the food they get, to draw further inferences, even if we knew what work on a level the movements of their laden body represents.

On the whole, we come to the conclusion that animals, to perform work, *constantly* use not only a larger quantity of food, and therefore proportionately more albuminous matters, but that they further require for their labour a more highly albuminous diet. In this way the body attains a definite stationary condition, and that food is thus consumed and used. Consequently in the excreted matters the same quantity of nitrogen must occur as in the ingesta. The nullity of the argument derived from the slightness of the increase of the urea excreted during the performance of work is thus again made apparent.

XI. THE FOOD OF MAN.

We have approached the great question :—What food does man require? We may put this question generally. It is then not simply with what food is man best adapted for work, but rather, under what relation of the alimentary principles is he capable of the greatest perfection in mind and body. If science can here pronounce no definite opinion, neither has experience solved the problem. Where we, whether in the same people or in people of like race, meet with a great difference, in general not only is difference of food connected therewith, but contrasts exist in many circumstances of a material and moral nature, and our knowledge fails to isolate and estimate the influence of each circumstance. are, however, not without a leading principle. One fact is established, to which we attach great importance, that man everywhere exhibits a tendency to the use of mixed food, and under its use is well developed and in good health. This fact is, in truth, of much significance. It speaks to us of our ancestry. It teaches us under what circumstances man has become what he is. With the food which he used, his system must have been in harmony, and his inclinations, too, are connected therewith. Many years ago I had shown that the harmony of animal life in general finds its explanation in three laws—that of habit, of exercise, and of hereditariness. The first announces that each animal being is so modified in its organization by the influence to which it is persistently exposed that it harmonises with these influences. The second is, that each organ, each part of the body is so modified under the constant influence of the will or of other circumstances which determine its action, that it corresponds to what this will or these circumstances require of it. And finally, the law of hereditariness is thus expressed: that the peculiarities of ancestry are transmitted to posterity; this law, therefore, confirms for the subsequent generations what habit and practice have effected in the preceding ones. By these laws, which we find everywhere confirmed by the phenomena,[a] all that we call suitable in nature is recognised as necessary; harmony is only a manifestation of these laws. And if we further reflect that defective objects often die prematurely, without propagating their race, and that also in the remaining ones the conditions for propagation are in general less favourable, we find in the laws described at the same time the foundation of progressive perfection in creation. But is retrogression, then, inconceivable? By no means. If the conditions whereby the nature of organic beings is determined, are no longer to be realized, the race is threatened with retrogression, which may terminate in destruction. So man is also, under the influences of over-population and accumulation in great centres, liable to retrogression. Want of the aliments, under the use of which man has become what he is, may be a very essential factor therein.

On another occasion[b] I have proposed to myself the question:—What, after a series of generations, would become of man if he gradually accustomed himself to an exclusively vegetable or animal diet? I acknowledged that the results thereof could not be foreseen, but at the same time I showed that it would be a rash struggle of the human will against the course of nature to strive after another harmony, whereby, even if it could be effected, its best qualities and noblest gifts might easily be imperilled. Now from time to time some, impelled by certain philosophical or religious prejudices, have advocated the exclusive use of vegetable food. Perhaps such a doctrine should be opposed with some earnestness if we were not convinced that it will never meet with much countenance. Nature is stronger than theory. To the followers of Pythagoras, who rejected the use of animal food, my friend the late Professor Karsten thought that the "naturam expelles furcâ, tamen usque recurret" might also be safely applied. Meanwhile, wonders are related of the sect of the *Vegetarians.* The children of their first apostle, a certain John Newton, who in 1811 made his appearance with his doctrine, were said to excel in health and beauty, and to be perfect models for a sculptor. If the statement is true, is it nevertheless not absurd to connect the beauty of the bodily forms with the diet followed in a single generation? Let natural science beware of extending the hand to anomalies of the religious sentiment.

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[a] More fully developed in my inaugural address: De harmonie van het dierlijk leven de openbaring van wetten. [The harmony of animal life the manifestation of laws.] Utrecht, 1848.

[b] De voedingsbeginselen. [The principles of nutrition.] 1858, p. 40.

We may now further inquire:—What does daily experience teach respecting the more direct influence of diet? We have thus far spoken of vegetable and animal food. Evidently, however, the question refers only to the proportion of the alimentary principles. In general we may say that fats, and especially albuminous matters, are more fully represented in animal than in vegetable food. But fats are fats, and they will not differ in their action on the animal system, whether they be of vegetable or of animal origin. The same is true of albuminous matters. Therefore, too, if there be any difference, for example, between the use of bread and butter and of meat and potatoes, this will be found rather in the accompanying alimentary principles than in the essential necessary elements—albuminous matters, fats and carbo-hydrates—it being understood that these, in both cases, are present in equal proportion.

Now, experience teaches that a vegetable diet, with excess of carbo-hydrates, promotes the deposition of fat in the body. The animals we fatten are all herbivorous; and for a particular species, too, such as the pig, which belongs to the omnivora, potatoes are quite enough. Even dogs and cats become stout and fat on vegetable food—a proof that it is not so much the kind of animal as the nature of the food which makes the difference; and, just as any other carnivorous animal, they always become, on the most liberal supply of meat (of course without much fat) comparatively lean. In accordance herewith we see excessive deposition of fat in man, too, disappear through the use of animal diet. William Banting,[a] like many before and after him, has experienced the truth of this in his own person; and the experiments of Ranke, communicated above (p. 36) supply the key to it. Now, if it be true that in the animal system fatty matters are formed not from carbo-hydrates, but precisely from albuminous matters, as becomes more and more probable, it is sufficiently evident that the formation and deposition of fat are connected with very different conditions.

From our remarks, as well as from the facts adduced, we arrive at the conclusion that all men ought not to be fed in the same manner. Their diet differs in different nations, partly in connexion with the climate they inhabit, partly in connexion with what the country yields, and by mutual interchange both parties would lose. In the same people, too, the feeding must vary according to birth, position, calling, and habit. Even the requirements of the individual must be attended to. With a tendency to excessive deposition of fat, which interferes with

[a] This William Banting, a well known and esteemed inhabitant of London (formerly "a furnisher" of the "fashionable world"), in a "Letter on Corpulence, addressed to the Public," London, 1863, describes with as much simplicity as unmistakable good faith, the favourable influence which animal diet speedily had on the excessive deposition of fat which had for years oppressed him.

many functions, albuminous matters should be freely given, while the use of fats and carbo-hydrates should be limited, and the latter, provided they are well borne, may be advantageous to the lean. An intelligent man will in this respect consult his own experience.

In particular we have finally to ask:—On what diet is man best adapted for muscular work? In connexion with this inquiry I cannot suppress the remark, that in my opinion a too absolute value has been attached to capability of muscular work. It was as if man was to live exclusively for his muscles, and as if, with the acquirement of the greatest muscular power, the most perfect health and the highest development in every direction would also be attained. This partiality has also sought advantages in gymnastic exercises, which I cannot possibly find in them, unless, perhaps, as corrective of the injuries proceeding from constrained sedentariness in schools, against which the natural movements of running, leaping, and wrestling may do quite as much, without giving rise to disproportionate and consequently unpractical muscular development. We cannot overlook that with the increase of civilization the muscular work required of the people gradually diminishes Everywhere the tendency is to replace this by machine work, for the very valid reason, that mechanical work is under no form so costly as under that of muscular work, especially of men. It is true that we obtain, in reference to the fuel (food) consumed, more useful work than in the best steam-engine;[a] but the fuel is amazingly dear in comparison with the price of turf and coal. Thus we should be able to justify the proposition that the worst use to make of a man is to employ him exclusively in mechanical work.

But the question:—On what food man is best suited for mechanical work has in this way by no means lost its importance. Muscular labour is still connected with all work, and in some kinds of work powerful exertion of the muscles is always involved—digging, ploughing, and thrashing, the hoisting or carrying up of heavy loads, the handling of heavy hammers or hatchets, rowing, sawing, ramming, &c.; also long marches, with bag and baggage, and various exercises in the management of arms, are to be enumerated. Moreover, muscular work, in a certain measure, will always be required in the interest of the organism. That albuminous matters are in the first place necessary for this purpose, and that they should be more liberally represented in the food of the working man than is the case with us, was some years ago argued by Mulder with an enthusiasm which was the result of a deeply-felt conviction. This conviction communicated itself to all. On no side was it opposed. It was not asked whether muscular work was produced directly from the albuminous matters—the law of the maintenance of energy had as yet found but little application: but he reasoned simply thus: the muscles must perform work, and they must therefore, in the first instance, be well nourished, and indeed with albuminous matters, of which they are chiefly composed. Now, after it was on the one hand proved that non-nitrogenous matters were also capable of producing mechanical work, and after it was on the other hand shown that in this work a proportionate metamorphosis of albuminous matters does not always take place, some began to abandon their conviction. And soon they now came forward with the assertion that heavy work is performed with a slight supply of albumen, and that it is precisely the consumers of albumen who in general use their muscles but little. The last statement is perfectly true; it proves, however, as we have already said, only that also without muscular work much albumen may be metamorphosed. The first we deny. Science has led us to infer that a supply of albuminous matters makes the muscles better adapted for work, and daily experience fully confirms this. The argument made use of we reject for the following reasons:—

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[a] Conf. Helmholtz, Lectures. *L. c.* Lecture vi.

1. People are deceived as to the amount of work performed. Most callings require rather an accurate use than great tension of the muscles. In fact the work performed by the human hand is, when measured by the accurate standard of kilogrammeters, in general very little. If the manufacturer of fabrics was called to perform hard work, to carry heavy loads, or to overcome great resistance, it would soon be seen how little he is fitted for the purpose. Even his very appearance betrays it. On slender food and under unfavourable circumstances his physical condition is lowered to a degree at which it can now be maintained by that slender diet. He gets little, especially of albuminous matter, and has scarcely need of more (compare p. 35). At the same time he has lost the bodily form, the shape, the weight even of the normal man. For him special statistics exist. Thus he has the true capability for working in his manufacture. Involuntarily he appears before our mind's eye when we read in Bruecke:—"Scantily nourished on thistles and weeds, the ass has become the slave of the poor man, who feeds his beast by the wayside: he is the *proletary* among the beasts of burden, in whose degraded form we no longer recognise the strong and beautiful animal who formerly, decorated with costly trappings, bore on his back the princes of the East;" and the "quantum mutatus ab illo" is on our lips as we think of the old Batavians.

2°. They who actually perform work also use albumen, and in general consume more in proportion to the severity of the labour *a.* The quantity of food is great, surprisingly great, we will say, if we visit the tables of the country people, who do heavy work. Proportionate thereto is the albumen ingested. *b.* They get more albuminous matters than one suspects. Besides potatoes they use different kinds of flour and bread in great quantity, and every evening trencherfuls of buttermilk, which contains all the casein of the milk; and in the middle of the day pork and bacon. For soldiers in garrison Mulder calculated 100 grammes of albuminous matters daily; sailors receive still more. *c.* In a yard visited by me some men were employed the greater part of the day sawing planks. The strongest were chosen for it. They got higher wages and required it, for "without a pound of meat daily and a good jug of beer one could never hold out at that work." Thus, too, the guide, who daily ascends mountains, helps the traveller and carries his valise, must be well fed and especially requires animal food. Of the English labourer, who daily gets meat and drinks strong beer, we know that he is strong and active, and the Irish people, who live almost exclusively on potatoes, are called lazy and slothful. I have often remarked that slothfulness is rather a morbid symptom than a vice.

The conclusion is this:—Muscular work and heat arise in the animal organism, both being derived from the chemical energy as well of non-nitrogenous as of nitrogenous matters. Of both kinds of food the animal system has need. In the body there exists a certain relation between heat produced and muscular work. By exercise this relation becomes more favourable for muscular work. A liberal supply of albuminous matters tells favourably in the same. The reason of this is probably to be found in the better nourished and firmer condition of the muscles and of the whole body, which is obtained by means of a more highly albuminous diet. The development of man in general appears to attain the highest pitch under the use of a mixed diet.